EDITORIAL

THE December number of the *Astrophysical Journal* gives a translation of a paper "On the Constitution of Gaseous Celestial Bodies," by A. Ritter, which possesses much geological significance if its general conclusions are trustworthy. The original paper is one of a series of eighteen which appeared between the years 1878 and 1883 in Wiedemann's *Annalen*, but its astronomical and geological bearings appear to have escaped the attention they merit, and for this reason it is now reproduced. Ritter attempts to compute the time which would be occupied by a gaseous solar sphere of the dimensions of the earth's orbit in contracting to the dimensions of the present sun; in other words, the time of evolution of the solar system from the separation of the earth to the present stage under the Laplacean hypothesis, with certain qualifications. The computation is necessarily based on certain assumptions, some of which require modification in the light of more recent investigations, but any competent attempt at a mathematical discussion of the rate of solar evolution under the gaseous hypothesis constitutes a notable contribution to the cosmical phases of geology. The conclusion is reached that about 5,500,000 years ago the solar radius was equal to the radius of the earth's orbit. On the assumption that the effective radiating disk of the sphere was always equal to the whole disk, Ritter concludes that the solar mass shrank from dimensions of the earth's orbit to a dimension ten times the present sun's diameter in the remarkably short period of 255,710 years. On the assumption that the effective radiating disk was half of the whole disk, he finds that a similar shrinkage would take 511,420 years. In the latter case the total time occupied by the solar mass in contracting from the earth's orbit to its present dimensions would be about 5,765,000 years. This con-

clusion is based on the assumption that the thermal capacity was 1.41. On the assumption that it was five thirds, which is the largest permitted by the mechanical theory of heat, the conclusion is reached that the contraction could at most have occupied about 6,500,000 years.

The foregoing computations are based upon Pouillet's estimate of the present radiation of the sun. If the computation be based on recent estimates, which give a rate at least 50 per cent. greater, the resulting time is about 4,336,000 years. Ritter recognizes that the departure of the body from a spherical shape arising from rotation would modify the results, as these were based on the assumption of a spherical form throughout the whole period, but as this departure was large only during the comparatively small portion of the whole interval occupied in the contraction from the earth's orbit to twice the sun's present diameter, the correction is limited, but yet may be considerable. In view of this the author remarks: "For these reasons we cannot give the maximum value $t=4{,}336{,}000$ years found above the significance of a superior limit for the age of the earth, the less in fact since the original assumptions must still be regarded as hypotheses imperfectly satisfied. Nevertheless it seems permissible to conclude from the above investigation that the actual age of the earth must be far less than the estimates of some geologists, who place it at hundreds of millions of years."

Whatever corrections may be applicable to such a computation, the attempt to subject the time factor of the gaseous hypothesis of the evolution of the solar system to rigorous mathematical inquiry is a most helpful one. The discussions of Lord Kelvin and others who have attempted to assign limits to the age of the earth by merely determining the maximum amount of heat which the sun can have radiated in the past, on the gravitational hypothesis, do not really get home to the question, since they do not determine the rate of radiation of heat in the past. If that rate were faster than the present rate it is obvious that the time would be correspondingly shortened; if

slower, it would be correspondingly lengthened. This radical defect is obviated, in the main, by Ritter's method.

If the period occupied by the supposed gaseous ancestor of the sun in shrinking from the earth's orbit to its present size is such as computed by Ritter, or if it be any period of that order of magnitude, it will probably be the conclusion of geologists and biologists that the hypothesis of such a gaseous sun is irreconcilable with geological evidence and with the phenomena of biological evolution. At any rate, this is a mode of testing the validity of the gaseous hypothesis which merits the careful consideration of those competent to pass judgment upon it, and it is earnestly to be hoped that the method of Ritter and his assumptions will be subjected to critical reëxamination in the light of the most recent researches.

The press announce that in a recent lecture before the Lowell Institute, Dr. See stated certain radical conclusions which he has reached with reference to the temperatures of the exteriors of gaseous bodies. We understand that his fundamental formula is closely analogous to one of those derived by Ritter. Applied to the sun when expanded to the dimensions of the earth's orbit, it gives a relatively low external temperature. It is not clear that this low outer temperature is compatible with the rapid loss of heat that appears to be involved necessarily in Ritter's rapid evolution, and we do not understand that Dr. See holds the latter view. Geologists will watch with interest the appearance of Dr. See's new views in authentic form, and may well congratulate themselves on the prospect of a discussion of the nebular hypothesis on new lines.

T. C. C

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THE eleventh annual meeting of the Geological Society of America, which was held at Columbia University, in New York City, was characterized by a large attendance of the Fellows and by a very general interest in the proceedings. The accommoda tions furnished by the University were sumptuous in many ways the elegant Schermerhorn building proving highly satisfactory

except for the acoustic properties of the large lecture hall. The opportunities for luncheon were adequate and agreeable. The social features of the meeting, consisting of receptions at the American Museum of Natural History and at Professor Osborn's residence, and the annual dinner, were eminently successful. The dinner was pronounced the most satisfactory yet enjoyed by the society.

The program was varied and attractive, no one branch of the subject being greatly in excess of others. General geology, stratigraphy, physiography, glacial geology, palæontologic geology, and petrology were each represented by able exponents. But to those who attempted to follow the programs, it was evident that the shortness of the time devoted to the meetings, together with the length of the program of a well attended session, necessitate a better regulation of the proceedings than it has heretofore been the custom of the presiding officers to enforce. The evident hesitation on their part to interfere with the presentation of papers by Fellows of the society, while agreeable to the individual at the time, is not conducive to the best interests of the society as a whole, that is to say, to the other Fellows in general. Interference may properly be exercised in the case of those who exceed the time allotted them for the presentation of papers, especially since in most instances the time is that determined by themselves.

There should also be some rule limiting debate both as to length and matter. The exhibition of lantern views is a most valuable aid to the presentation of many subjects, which was very well shown at the meeting just held, but the selection of illustrations should be limited to those which actually illustrate the subject, and should be made to avoid unnecessary repetition.

The result of these abuses, the overrunning of time in presentation and discussion, and the introduction of unnecessary illustrations, is the crowding of papers on the last day of the meeting, the consequent haste in their delivery or the curtailing of considerable parts of them, and a general sense of dissatisfaction; first, with those who said too much, and last, with those

who said too little. The correction of these evils should rest with the presiding officers, but must originate with the Fellows themselves. It is to be hoped that some regulations will be formulated and put into operation at the next winter meeting in Washington, D. C.

J. P. I.

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In an article on igneous intrusions in the October-November number of this Journal, Professor Iddings states that, "Russell has called attention to what he considers volcanic plugs in the region of the Black Hills of South Dakota." I desire to deny the statement that the intrusions referred to were called *volcanic* plugs; abundant evidence was, I think, presented to show that they are, as I termed them, *plutonic* plugs. They are intrusions of igneous magmas forced upwards into horizontally stratified rocks so as to raise domes above them; but did not reach the surface, and hence should not be considered as occupying the conduits of volcanoes, and so far as can be judged, did not expand laterally after the manner of laccoliths. Not only one such intrusion was described, but several in various stages of exposure by erosion, from an unbroken dome of stratified beds, presumably with an intruded plug beneath, represented by Little Sun Dance Hill, to the imposing fluted column of Mato Tepee, over 600 feet high. Associated with these plug-like intrusions are what appear to be true laccoliths, as Warren Peak, for example. When this instructive region is more thoroughly explored, we may expect to find a series of examples illustrating the transition from plug-like to cistern-like intrusions or laccoliths. For these reasons it is well to hold the locality referred to, as furnishing the type-group of plutonic plugs.

The evidence just referred to was stated in the article[1] criticised by Iddings, but without having seen the intrusions and without presenting any new observations concerning them, he brushes it aside and restates the same kind of evidence for the

[1] Igneous intrusions in the neighborhood of the Black Hills of Dakota, Jour. Geol., Vol. IV, 1896, pp. 23–43.

apparent purpose of introducing a high-sounding Greek name in place of the term used by me.

In discarding the evidence of the plug-like character of the intrusions near the Black Hills, Iddings states that it is probable they are central remnants of small laccoliths, for the reason that the prismatic columns of which they are largely composed are vertical, "whereas they should be horizontal in the body of a volcanic plug." Unfortunately for this dictum, the prisms in many true volcanic plugs or necks, like those about Mt. Taylor, New Mexico, described by Dutton, are vertical.

In the same spirit in which Iddings discards the evidence of the plug-like form of the intrusions under consideration, and with equal justice, one might use his own language in reference to the account he himself gives of Mt. Holmes, the new type-example brought forward and of the accompanying, largely ideal diagram; "He has mentioned nothing that demonstrates or even indicates that it possesses the character of a plug;" it might just as well be a laccolith eroded down to the feeding conduit.

The term *bysmalith* which Iddings seeks to substitute for plutonic plug, means simply plug-stone, and may be used with equal propriety for both volcanic and plutonic intrusions, of plug-like form; if one wishes to make this convenient distinction the terms *volcanic bysmalith* and *plutonic bysmalith* would have to be used. I fail to see any advantage in such a clumsy nomenclature. The word bysmalith is so similar to *bathylith*, already in the field and also used by Iddings in the article referred to, that confusion must arise if this rechristening is permitted. It seems to me that American geologists should use their mother tongue whenever it can be made to serve, and usually it will be found rich enough to express all the ideas they may have, instead of searching the dictionaries of the dead languages for more or less accurate translations of plain English terms.

ISRAEL C. RUSSELL.

[The use of the term volcanic instead of plutonic in referring to the intrusions in question was inadvertent; much of the assumed distinction in the use

of the terms being artificial and misleading, the writer has become indifferent in his use of both terms.

Nevertheless, with regard to what Professor Russell has called plutonic plugs in the Black Hills region, it may still be said that "In his description of them he has mentioned nothing that demonstrates or even indicates that they possess the character of a plug. In each case they may be central remnants of small laccoliths."

The question, whether the evidence regarding the nature of the Holmes bysmalith is of the same kind as that offered for the character of the Black Hills intrusions, may very well be referred to our fellow-geologist. J. P. I.]